

CPC

Climate Policy Center



www.cpc-inc.org

HOUSE COMMITTEE ON GOVERNMENT REFORM

**“CLIMATE CHANGE TECHNOLOGY RESEARCH: DO WE
NEED A ‘MANHATTAN PROJECT’ FOR THE
ENVIRONMENT?”**

Thursday, 21 September, 2006

Testimony By
Lee Lane
Executive Director
Climate Policy Center

Introduction

My name is Lee Lane. I am the Executive Director of the Climate Policy Center, a non-profit, bipartisan Washington DC-based policy research organization seeking to analyze climate policy options and to promote economically efficient policy responses to the challenge of climate change. CPC is supported primarily through grants from non-profit foundations.

To begin, I wish to thank Chairman Davis and the House Committee on Government Reform for calling this hearing. For reasons that I will explain, I believe that in doing so the Committee is posing the single most important question in climate policy – how we can best accelerate progress toward technological solutions to climate change. Within that larger, question a focus on exploratory research and how to organize it also seems to me to be precisely on target.

My testimony this morning will begin with an explanation of why making organizational improvements to the US Climate Change Technology Program (CCTP) is extremely important to the prospects for long-term success in coping with the challenge of climate change. Then I will describe three near or mid-term steps that could potentially improve the existing program. My three candidates for prompt action are as follows:

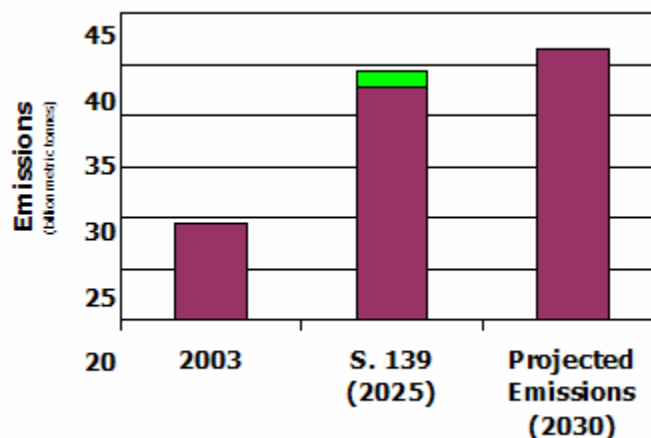
1. Create a new entity to conduct government-funded research aimed at making high payoff scientific and engineering advances capable of dramatically reducing the potential future harm from climate change.
2. Conduct the R&D needed to make more informed choices about the potential pluses and minuses of various geoengineering responses to climate change and to lower the costs of adapting to climate change.
3. Congress needs to provide the management of the Climate Change Technology Program (CCTP) and to ensure that its recommendations and analysis receive due attention in future budgeting and organizational decisions about CCTP.

Government-funded R&D, a key to climate policy

Cost-effective government funding for R&D will be essential in meeting the challenge of climate change. Without truly revolutionary technological advances, comprehensive Greenhouse Gas (GHG) emission limits are likely to prove ineffectual in the industrialized world and not to be implemented at all in China and India.

Although the industrialized countries, including eventually the US, may adopt GHG controls, with current technology – *and given realistic assumptions about social willingness to pay for GHG abatement* – such controls will do little to curb the growth in global emissions. Thus, according to the US Energy Information Agency's estimate, by 2025, even the original version of S. 139 would have reduced global GHG emissions by a paltry 2.6 percent. To illustrate the point, Figure 1 shows the estimated emission reductions from the original version of S.139. It compares those hypothetical emission cuts with projected global GHG output and growth. Of course, for a still higher cost, controls could more tightly limit emissions. Yet the sponsors of S.139 withdrew this bill in favor of a less ambitious version. They took this step because they judged the initial bill's high costs as politically unacceptable, and even the scaled-back version has failed twice to attract majority support in the Senate.

Figure 1



In theory, more could be accomplished if China and India could be induced to curb their GHG emissions. But with current abatement costs, those countries seem most unlikely to institute GHG controls. Both the Chinese and Indian governments are under tremendous pressure to rapidly industrialize their countries. Strong GHG control measures would impede industrialization. These governments feel little domestic pressure to halt climate change. Even with great technological progress, the task of lowering Chinese and Indian emissions will be difficult and time consuming. Without new technology, the hopes of significantly limiting Chinese and Indian GHG emissions seem largely fanciful. (Even with a good deal of technological progress, this goal may be difficult to reach.)

Some proponents of GHG cap-and-trade policies argue that imposing controls would stimulate enough private sector R&D to produce the technological innovation needed to drive down abatement costs. While not entirely unfounded, such hopes are often exaggerated.

To a degree, placing a price on carbon emissions must occasion some change in the pattern of private sector R&D. However, GHG limits' induced-innovation effect will be small. Almost all economists recognize that market forces call forth a less than the optimal quantity of R&D. Once a private sector innovator demonstrates a new technology's feasibility and profitability, competitors are likely to imitate it. The imitators would escape paying the high fixed costs required to make the original discovery. Therefore, copycats could gain market share by undercutting the innovator's prices. By doing so, they deprive the initial developer of most of his hypothetical financial gain. Foreseeing this competitive response, firms avoid investment in many R&D projects that might have been profitable had the innovator been able to capture the full reward of his success.

The private sector's reluctance to rely on R&D strategies is likely to be especially strong with the kind of R&D activities needed to reduce GHG emissions. No large inexpensive emission-free energy sources lie just over the horizon. Thus, successful innovation in this area will require unusually high risks and long delays. As many economists (most recently Edmonds and Stokes) have noted because developing these technologies will entail breakthroughs in basic science, much of the most essential work will be ineligible for patent protection. These are precisely the conditions in which firms are least likely to invest in on R&D as an approach to problem solving.

I do not wish to overstate this case. Putting a price on GHG emissions would certainly encourage private sector R&D designed to commercialize new technology. Indeed pricing emissions is

almost certainly the most cost-effective way to encourage such commercialization. Nonetheless, as the recent editorial of the journal *Nature* noted, government funding must play the central role in making the scientific breakthroughs without which no long-term climate policy success will be possible. (See Attachment A)

Climate Change Technology Exploratory Research

If only one change in US government climate policy could be made, it should be to initiate and sustain a properly organized exploratory research program like that described in Attachment B. Attachment B is a paper drafted by four accomplished scientists and me. It describes one possible ‘straw man’ proposal for how such an exploratory research program might be structured. My colleagues and I dubbed the proposed program Climate Change Technology Exploratory Research (CCTER).

Such a new program would conduct R&D aimed at fostering potentially high payoff (even if high risk) technological solutions to climate change. The key problem that this proposal is designed to address is the rigidity of the government’s climate change technology R&D and the high entry barriers that often prevent innovators from gaining government financial assistance for testing and developing their ideas.

In particular, the existing program has seemed to slight longer-term potentially big-payoff high risk ideas. The organizational culture of the Department of Energy (DOE), its energy industry constituencies, and the legislatively mandated requirements for industry partnerships virtually ensure temporal myopia in DOE’s research agenda. But the short-run focus clashes with the long-run climate policy vision. In the words of the report summarizing the conclusion of the expert workshops that recently reviewed CCTP’s draft strategic plan:

The need to place more emphasis on long-term, revolutionary technologies as well as programs that foster truly innovative, unconventional approaches emerged as essential for meeting the century-long challenge of climate change. This might require a greater percentage of funding to go to potential breakthrough technology versus research that provides only incremental improvements. There is a need for more high-risk but high payoff research. (Brown *et al.* xi)

Without structural change, the more innovative longer-run orientation seems unlikely to receive the optimal attention or funding.

As one possible solution, my colleagues and I proposed creating a new organization that would provide seed money to a number of innovative concepts that fall outside the current R&D portfolio of CCTP. Eligible concepts might be classic ‘Pasture’s Quadrant’ research, *i.e.* basic research with a high potential to advance technologies relevant to coping with climate change. They may also be more applied research.

The key criteria would be that the concepts not duplicate CCTP’s current work and that their success would make a large improvement in the costs of halting climate change or coping with it. CCTP might not explore meritorious R&D concepts because they are more risky or more long-term; because they are narrowly focused on climate; because they are too multi-disciplinary or cross-cutting; or simply because they are too innovative. In all these cases, CCTER would be a possible alternative source of seed money. The Defense Advanced Research Projects Agency

(DARPA) experience seems to suggest that launching a systematic search for technological solutions that we cannot initially name or clearly describe can produce amazing results.

As we conceived it, CCTER's internal mode of operation would resemble that of DARPA. CCTER would adopt a flat organizational structure. Recruiting high quality staff would be a priority as would limiting the influence of purely bureaucratic constraints. It would use grants, contracts, and in my view prize competitions to produce research. It would not conduct intramural research – or very little. Competition through peer review and (perhaps) inducement prizes would determine specific financial rewards. (I do not recall that we actually considered inducement prizes in the discussions surrounding the preparation of our paper, but it is a tool that I personally believe deserves to be included in an exploratory research program.) Solicitations for potential grants, contracts, and awards would be broad, ideally worldwide.

Annual steady state funding would be at the level of \$35-50 million. This number comes with the caveat that most of us thought that eventually higher expenditure levels would be desirable although we were conscious of the need to ramp up spending cautiously. One advantage of more funding would be that it would allow more opportunities to demonstrate the promise of technologies before requiring that further support come from traditional government or private sector R&D programs.

Our hope is that the private sector would supply some of CCTER's money. This would also keep CCTER personnel in close touch with market realities. However, I personally believe that the program's *raison d'être* as a search for unconventional and highly innovative technologies would probably hold private sector investments to fairly low levels.

One key organizational question is whether the goal should be to create CCTER or an Advanced Research Projects – Energy (ARPA-E). In principle, if an ARPA-E were created in DOE, it could be charged with climate-related exploratory research. This arrangement would entail some risk of losing the program's climate change focus amidst more pressing issues of energy price and security. In any case, for the next two years, the Administration's opposition to the ARPA-E concept effectively removes this option from the table. Under the circumstances, only a more narrowly climate-focused program like CCTER may be feasible.

Determining how to fit CCTER into the larger structure of government is harder than envisioning how it might function internally. The option on which we settled was a private non-profit corporation. It would be governed by a board of directors that would include representatives of both government and the private sector. Government financial support would be funneled through DOE, but CCTER's governance arrangements would insulate it from bureaucratic interference.

Other options that we considered were to create an exploratory research program within DOE and to create a federally-funded R&D center under DOE. For my part, work by Dr. Van Atta on DARPA's history suggests that an innovative high risk R&D operation cannot survive and prosper within a federal bureaucracy without a high degree of top management support. The current views of DOE toward exploratory research and the concept of an ARPA-E raise serious doubts about whether that Department would provide a suitable environment for this kind of enterprise.

Back-up strategies for climate change

Currently, CCTP is focused exclusively on technology that would reduce GHG emissions. The program explicitly excludes R&D on geoengineering and adaptation. (US CCTP [Draft] Strategic

Plan 2-2 note 2) This decision does not appear to have received an appropriate level of attention. To underscore this point one recommendation of the expert workshops conducted in the wake of the draft plan's publication was to expand the program's agenda to include geoengineering. (Brown *et al.* xi)

Broadly considered, three technology strategies are available for dealing with climate change. First, technological progress can lower the cost of GHG abatement. CCTP focuses on this goal. Second, technology can enable what is called 'geoengineering', use of technologies that would avoid harmful climate change even though emissions continue at relatively high levels. (Among other options, geoengineering could involve increasing earth's albedo (the proportion of sunlight striking the earth that is reflected back into space) to offset the warming effects of rising GHG concentrations.) Third, technological innovation could aid adaptation. Technologies like heat and drought resistant crops, stockpiling genetic material from endangered species, or hydrological projects that minimize the costs of rising sea levels can minimize the costs of the climate change that is already inevitable.

Some mix of the three technology strategies is most likely to meet the goal of minimizing the sum of the costs of climate change and the costs of countermeasures taken against it – the definition of an optimal climate policy. CCTP should develop the suite of technologies best able to implement this cost-minimizing strategy. Each of the two missing strategies is, in fact, potentially crucially important to overall success.

The fact is that some degree of human induced climate change is already inevitable. It is hard to be certain whether it will be a lot or a little or what affects it may have. But as one looks farther into the future, the chances of larger and more disruptive impacts grow. To many scientists the preferred policy response to these problems is to restrain GHG emissions so as to minimize the future risks. However, to some degree, as the experience with the Kyoto Protocol suggests, such efforts have already failed. Furthermore, some prominent economists who have studied the difficulty of forging an international agreement on GHG controls conclude that it may be impossible to do so, at least for several decades. If they are right, and so far the Kyoto Protocol experience suggests that they are, a substantial degree of climate change is inevitable.

Adaptation could significantly reduce net damages from this climate change. Many recent climate change damage estimates typically fall below those of earlier studies because the more recent studies have better accounted for adaptation. (Joel B. Smith 31) Adaptation's evident power to decrease damages suggests using R&D to boost that power.

It [adaptation] means *inter alia* pushing ahead with both the basic science and applications of genetic engineering in many areas, especially agriculture, but also to provide potential substitutes for possible useful species that may be lost. That could be supplemented by a systematic program for collecting, cataloguing, and storing genetic material, mainly but not exclusively from plants, in the form of seed banks and DNA. (Cooper 1999, 43)

Other relevant work might involve a systematic study of how public and private infrastructure might be adjusted to minimize the costs of climate change. It may even be worth exploring adaptation's role in US assistance to foreign countries.

Taking the logic of adaptation one step further implies conducting R&D on geoengineering. Climate policy must cope with the possibility of low probability but high cost events. (Nordhaus & Boyer 98) Should the climate system manifest a large and harmful discontinuity, having a mechanism for ‘scramming’ the climate change process could prove invaluable.*

Indeed, unless we are prepared to assign a zero probability to “nasty surprises” from climate change, there seems good reason to undertake such research. (Keith and Dowlatabadi 293)

Geoengineering in the climate change context refers mainly to altering the planetary radiation balance to affect climate and uses technologies to compensate for the inadvertent global warming produced by fossil fuel CO₂ and other greenhouse gases. An early idea was to put layers of reflective sulfate aerosol in the upper atmosphere to counteract greenhouse warming. Variations on the sunblocking theme include injecting sub-micrometer dust to the stratosphere in shells fired by naval guns, increasing cloud cover by seeding, and shadowing earth by objects in space. ... Climate model runs indicate that the spatial pattern of climate would resemble that without fossil fuel CO₂. Engineering the optical properties of aerosols injected to the stratosphere to produce a variety of climate effects has also been proposed.” (Hoffert *et al.* 986)

As insurance against runaway climate change, research on geoengineering may be the only available option. Because emission reductions require so much time, a GHG control policy must be initiated many decades before it achieves its full effects – and long before the full extent of the problem is entirely visible. Our political systems are not always far sighted enough to respond to such long-term threats.

Geoengineering, in contrast to GHG controls, could be implemented swiftly. For one thing, reaching international agreement for geoengineering would probably be largely about the sharing of monetary costs, a type of negotiation for which we have much experience. (Schelling 2005 592) Meantime, the costs would be confined to the R&D needed to prove-up the technology’s feasibility. The future costs of actually deploying geoengineering solutions are highly uncertain; however, in the early 1990s, the U.S. National Academies of Science, after studying geoengineering, concluded, “Perhaps one of the surprises of this analysis is the relatively low cost at which some of the geoengineering options might be implemented.” (NAS 1992 460)

The logic behind an exploration of geoengineering is so strong that it is beginning to erode the taboos that have hitherto blocked its consideration. The newly-elected president of the National Academy of Science has become an advocate for exploring various geoengineering concepts. A growing number of other scientists, including Nobel Laureate Dr. Paul Crutzen of the Max Planck Institute have begun proposing possible approaches. Yet these scientists note the continuing absence of governmental support for the exploration of the various technologies. (Broad 1-4)

At this point, however, the pros and cons of the various approaches to geoengineering remain poorly understood. The cost, effectiveness, limits, side effects, and ancillary benefits are matters

* A ‘scram’ is the rapid emergency shutdown of a nuclear reactor or other system.

of growing speculation, but little empirical research. The social and political dynamics of geoengineering solutions have also not been systematically explored.

To some degree, CCTER would be a good place to conduct early R&D need to launch the exploration of geoengineering and adaptation (especially geoengineering). Nevertheless, there is really no good reason for CCTP not to add geoengineering and adaptation to its research agenda. Doing so would necessarily change existing funding patterns. However, early stage R&D in these areas is likely to be relatively inexpensive, so the adjustment would be small. After all, the whole point of CCTP and its strategic plan is to periodically retune the program's spending pattern to optimize the development of the portfolio of new technologies best able to cope with climate change.

More support for CCTP management

The long delays in the release of the CCTP strategic plan illustrate a persistent problem. The planning process has been starved for resources. The resulting nearly four year gap between the program's inception and the finalization of the first strategic plan is understandable, but disappointing.

Then too, in some important respects, the scope of the Draft Strategic Plan was undesirably narrow. To the authors' credit, they highlighted the omission of geoengineering and adaptation and solicited the comments of those who might disagree. Yet the only apparent rationale for excluding these areas was that the government was not in fact doing R&D in these areas.

This comment is not intended as criticism of the people who drafted the CCTP strategic plan or their decision to narrow the plan's scope. Given the resource constraints, that judgment was probably correct. The narrowness of the resource constraints, however, is just the point. Because planning resources were so limited, the planning process could not adequately address important issues.

CCTP may also lack influence on wider technology policy. The draft strategic plan correctly designates supporting technology policy as a CCTP "core approach". (US CCTP [Draft] Strategic Plan 2-10 – 2-11) However, last year's energy legislation authorized several new putatively climate-related subsidies and tax subsidies – several of questionable cost-effectiveness. Does CCTP have the resources and standing to conduct analyze these larger policy questions and to win a serious hearing for its findings?

Finally, what will be the significance of the plan now that it is approaching its final form? A recent National Research Council study of the Climate Change Science Program noted the importance of a "Leader with sufficient authority to allocate resources, direct research effort, and facilitate progress." It also called for "A strategy for setting priorities and allocating resources among different elements of the program (including those that cross agencies) and advancing promising avenues of research..." (NRC 80) These requirements are equally crucial to the success of CCTP. So far, though, if the issue of exploratory research is any indicator, CCTP has had little apparent influence on the distribution of DOE spending.

Conclusion

I do not wish to appear too negative about CCTP or about its strategic plan. The people working on drafting the strategic plan have performed yeoman service. While the plan is in some ways less than I would have wished, it puts the US vastly ahead of the rest of the world in establishing

a framework for its climate-related R&D. Many representatives of both European and Asian governments have privately admitted to me that they cannot even say how much their governments are spending on climate-related R&D, let alone articulate a coherent plan for the long-term future. The initial plan and our approximately \$3 billion in annual expenditures compare favorably with other efforts.

Moreover, the US has many pressing demands on our national R&D resources. National and homeland security, healthcare, the need for continued growth in economic productivity all come(s) readily to mind. Fiscal considerations, if nothing else, dictate that we may not be able to have all the R&D we would like. Given these competing demands, I would still wish to see climate-related R&D spending expand and several expert panels have also endorsed this idea. At the same time, it seems unlikely that climate relate R&D spending will burgeon to the unprecedented levels that some hope to see.

R&D cost-effectiveness is, therefore, likely to remain a high priority. I have proposed three steps to boost cost-effectiveness. These are, first, to create a climate-related exploratory research program outside of the DOE bureaucracy, second, to add preliminary R&D on geoengineering and on adaptation to the existing CCTP and third, to ensure that the management staff of CCTP has the resources to do their job and that their analyses receives due consideration in budgeting and management decisions.

By airing some of the potential problems with today's climate-related R&D and exploring the alternatives, this hearing contributes importantly to the search of climate policy solutions. It is an important step. And I commend Chairman Davis and the Committee for undertaking it.